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THE
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E. W. SCHAUFFLER, M. D., Editor.

CONTENTS:

ORIGINAL COMMUNICATIONS.

- Management of Placenta Prævia in Accordance with a new view of the Anatomy and Physiology of the Uterus—
By S. N. Denham, M. D. 7

ORIGINAL TRANSLATIONS.

- Therapeutics of Malarial Fever, 10
The Artificial Nourishment of Infants suffering from Intestinal Catarrh, 20

SELECTIONS.

- Transmissibility of Tuberculosis, 8
Remarks on Esmarch's Bloodless Method, 13

- Destruction of the Lung by Inflammation
Pneumia, 22
The Neuritic Origin of Diabetes, etc., . . . 20
Pneumonia by Streptococci, Successfully treated with a Monocerm and Carbazole Injection, 28
Angina Strictiva, 30
The Pathology of Vomiting, 31
Influence of Transient Changes on the Development of Scrophula, 42
Treatment of Diphtheria and Scarlet Fever, 44
Tuberculosis, 45
HYGIEINE, 34

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MEDICAL JOURNAL.

FEBRUARY, 1875.

**Management of Placenta Prævia in Accordance with a
New View of the Anatomy and Physiology of the
Uterus.**

BY S. N. DENHAM, M. D., KANSAS CITY.

I had not intended to inflict upon the readers of the JOURNAL an article on the subject of Placenta Prævia until I should have given, in another paper, some anatomical and physiological observations bearing directly upon the subject. Unavoidable circumstances have delayed my examinations in that direction, and I venture upon the present article at the risk of a little repetition at some future day.

Placenta prævia may well be ranked among the most formidable accidents to which the pregnant female is liable, and it may truly be said that science has contributed little or nothing for her relief. Why is it so? I verily believe that it is because anatomical facts, partially at least long known, have been neglected, and consequently physiological phenomena wrongly interpreted. What are these facts, and what are the phenomena and their rightful interpretation?

Without going into the full details of the anatomy and physiology of the female pelvic organs, let us notice some points immediately concerned in the process of gestation and labor, viz.: the peculiar force and action of the uterine muscular fibre—the constitution of the neck of the uterus and its obliteration during pregnancy and labor—the so-called descent of the womb after conception, etc.

If we make a longitudinal section of the vagina and unim-

pregnated uterus, and notice closely the distribution and course of the muscular structure, we may perceive something, so far as I am aware, not fully described by any anatomist;—a something which, if not a *deceptio visus*, will lead us to a correct understanding of the physiological dilatation of the os uteri. We shall find that a fibre, first noticed as a fibre of the vaginal wall, may be traced on upwards, doubling upon itself to form the uterine cervix, and continuing on to be lost in the body or fundus of the womb. Others may be traced from the body of the uterus down through the neck and into the vaginal wall or pelvic fascia. Let it be remembered that the muscular fibres never pass directly from the uterine walls to either the vaginal wall or pelvic fascia, but always pass through the cervix, which they chiefly constitute, by doubling upon themselves, and running back nearly parallel to the point where the vagina and pelvic fascia take a distinct course from the body of the womb. Thus it will be seen that the cervix uteri is constituted by two walls of muscular tissue, a process of connective tissue dipping down between them. We may, perhaps, get a clearer idea of the relation of the parts, by representing to the mind the vagina and uterus as one continuous canal, and then supposing an invagination to take place at the junction of the vaginal portion, the upper part of the viscus slipping into the lower, to the depth of the uterine cervix, and the two serous surfaces becoming adherent.

Now, if we follow up this idea a little farther, and look at the canal as constituted of threads or muscular fibres running longitudinally, we will observe that the neck or invaginated portion is represented by a doubling or loop in the thread or fibre. If we now apply to this thread or fibre, a force which causes it to contract or shorten, and the ends are fixed, the first observable effect is the reduction of the loop;—exactly what we see take place, in the neck of the uterus, during pregnancy, and under the contractions of labor.

Having thus rudely indicated the course of the muscular fibres of the uterus, and shown what some of the natural results of such a mechanism must be, we will next notice the so-called “descent,” or “subsidence,” of the womb after conception. So far as I am aware, it is admitted as a positive fact, by obstetricians and gynecologists, that “The whole body of the uterus sinks lower down in the pelvic cavity, and the os can be felt *lower down*,” in the early months of pregnancy. Such a state of things is literally a physical impossibility.

That in the course of its development, the anterior surface of the impregnated uterus, approaches nearer to the pubic symphysis, at the expense of the anterior wall, is true; but this is only a part of that delusive phenomenon which has led so many astray.

The uterus obeys the same physical laws that any other body would under similar circumstances, and neither ascends nor descends, unless impelled by physical causes. In the normal condition of the non-gravid uterus, the external os rests against the pos-

terior wall of the vagina, this wall being sustained by the perineal tissues, and hence cannot descend without bulging the perineum. Again, the utero-sacral ligament holds the organ back within a given distance of the sacrum, and will not allow its descent unless this ligament becomes elongated. Will anyone contend that such a thing occurs, during the early months of pregnancy? Certainly not.

What, then, is that condition, mistaken for descent of the uterus? We have seen that the organ cannot descend, so to speak, in the direction of its long axis, because the perineal tissues impose a physical barrier, and, we have also seen, that it cannot descend toward the outlet of the vagina, or the pubic symphysis, on account of the utero-sacral ligament, to say nothing of other causes. The increased diameter of the impregnated uterus requires additional space, and as it already lies in close proximity to the sacrum, it must extend itself in the opposite direction, or that of least resistance. This brings the anterior surface of the womb nearer to the pubic symphysis. Thus we have a developmental encroachment upon the territory of the bladder, instead of a simple anteversion, as generally supposed.

The cervix shortens as gestation advances, and finally disappears with the expulsive throes of labor; again to appear, as the womb contracts, and obliterates or closes its cavity after expulsion of its contents. Let us examine the manner in which this is effected. As soon as conception takes place, the uterine mucous membrane becomes hyperæmic, and the internal os closes more firmly than before. The os externum enlarges as gestation advances, and the uterine neck becomes flaccid; and if, during the latter part of pregnancy, the finger's end be passed into the open os, and carried up to the os internum, the latter will appear as a bead-like point, situated in a more or less tense wall, according to the situation of the patient. If now the free hand be placed upon the abdomen, and sufficient pressure made to produce a decided tension of the lower segment of the womb, it will be found that the neck is not affected, and that the finger can be carried to a considerable distance, around the internal os in every direction, pushing the fleeing walls before it, which give to the touch the idea of a loose cellular tissue; while the internal os remains a fixed point in the tense wall. At the close of gestation, the uterine neck has apparently disappeared. The internal os has assumed the level occupied by the os externum before conception, and thus while the uterus has not descended as a whole in the direction of its longest diameter, the os internum has descended, and now occupies the former line of the os externum, which has disappeared in consequence of the bulk of the muscular substance being drawn into the body of the uterus, through the enlargement going on in that part of the organ. There is still remaining, at full term, a portion of the cervix, which was originally represented, as it now is, by a line drawn from the junction of the vagina to the internal os. The obliteration of this

portion of the cervix, naturally dilates the os, and when complete leaves the vagina and uterus one uninterrupted canal. As soon as the uterine contents are expelled, and the womb closes its cavity, the neck again appears.

It may be asked, why the neck re-appears while contraction is present, as it has been stated that the obliteration of the same depended upon contraction? In reply we answer, when the contents of the body have been expelled, the organ continues to contract, until its walls come in contact, but now as the objects of resistance have been removed, the decreased size of the body no longer requires the entire length of the muscular structure, even after having reached its ultimate contraction, and therefore it resumes the natural form, aided in this by the recoil of the connective tissue, which naturally recoils, as soon as the high state of tension is removed, in which it was placed by the dilatation of the os.

If it be true, as above pointed out, that the hyperæmic condition of the uterine mucous membrane, which immediately follows conception, effectually closes the os internum, and the same remains closed until a late period of pregnancy, the location of a placenta directly over the os, becomes entirely possible.

As it may be supposed that the same thing is possible, and yet the current theory, or that of Smellie, be correct, let us briefly recall his doctrine, and see if such a thing be possible. Smellie believed that the dilatation began at the os internum, and gradually extended to the external os—that the cervical canal is dilated, and forms the lower segment of the uterine cavity at term. Now, as this would place the original point of the os internum one or two inches from the real os at the close of gestation, it has been argued that the placenta is never attached nearer than two inches from the margin of the os at full term. This idea is correct, so far as Smellie's theory is concerned; but neither of them will stand the test of reason or observation. According to the above, where would the maternal portion of the placenta take its origin? In charity to the above theory, let us admit that it does originate from the margin of the internal os, before the cervical canal begins to expand. We are told that the growth of the placenta is by marginal increase, therefore the small point of origin must be stretched to an extent of twelve inches in circumference, in order to cover the theory—a severe tax for the most elastic imagination.

In 1826, M. Stolz described the obliteration of the uterine cervix during pregnancy, as taking place from below upwards, which he believed to depend upon a process of softening of the circular fibres supposed to surround and close the os; this view has received the sanction of some of the most eminent modern physiologists and pathologists.

Both the theory of Smellie, and that of M. Stolz, make the vaginal portion of the womb entirely passive during labor. While the theory I have advanced, rationally explains the disappearance of the cervix, and its return after labor.

I wish fully to impress upon the mind the idea, that the remains of the uterine cervix is obliterated during labor, by the contraction of longitudinal muscular fibres, not peculiar to the neck, but belonging to the body also. That the same laws govern the actions of the cervix that govern those of the body of the organ. The same character of contraction dilates the os and closes the cavity of the uterus.

The first act of labor is a reduction of the size of the womb, by the contraction of the walls more firmly upon the contents, by which the woman experiences a feeling of "lightness," and the womb seems to sink, or subside into the pelvis—the latter is a delusion. This state of things is usually perceived by her, several hours before actual labor pains are announced. Once the labor pains have become decided, the contractions are seen chiefly to affect the cervix, and continue to do so until the uterine contents begin to protrude through the open os. This manifestation of contraction, alternated by relaxation, by which the os is dilated, affects all the free tissue of the uterus, but as the body of the organ cannot reduce its contents to a smaller compass, the further shortening of the uterine fibre, must take place at the expense of that portion contained in the cervix. So soon, however, as the reduction of the cervix has opened the os, so that the contents of the body can enter it, the organ can reduce its bulk, just in proportion to the exit of the contents from the cavity. This can be clearly illustrated by puncturing the membranes, and letting off the waters while the dilatation is slight. The whole body of the womb will then contract, *except that portion covered by the placenta*, but the contractions cease to be perceptible so soon as the contents can be no farther reduced, and again are chiefly manifested in the neck.

- I wish here to call especial attention to the fact pointed out above, viz.: that the portion of the uterine wall occupied by the placenta does not, in fact cannot, participate in the contractions. The detachment of the placenta must take place from its margin, and is a gradual process, either effected by contraction of the free portions of the fibres, or by other mechanical means. While its separation is easily effected, by attacking its margin, the force of the most muscular man would not be competent to detach it as a whole?

Are the above facts? If you doubt it, you can easily satisfy yourself by a little observation at the bedside. Now let us apply these facts to placenta prævia, according to the accepted view that dilatation takes place from above downward, and hence, for argument's sake, accept the doctrine that the placenta is attached two inches from the margin of the os in every direction. The attached portion of the placenta will then occupy a belt of uterine tissue, one and a half inches in width, supposing that body to be seven inches in diameter. Now, what will the natural phenomena be? First, a general contraction reducing the contents

of the uterine cavity to the smallest bulk. Second, dilatation of the os, by the contractions of the free segment of the uterine tissue below the placental attachment; this can occur to the extent of about four inches, before the placenta would be placed upon any great degree of tension, or its margin interfered with. Has anyone ever witnessed such a course as a natural one, in placenta prævia?

It may be objected that my theories are false, and therefore such would not be the natural course. In reply I will state that I have artificially produced the condition above represented, by detaching the placenta for some distance around the os, with the very result above stated. We also have a somewhat corresponding condition, in the so-called dry labor, where the waters have escaped from accidental rupture of the membranes, at the onset of labor or even before.

If the above statements are correct, then the course to be pursued in placenta prævia is clear. Detach the placenta in all directions sufficiently to allow the contraction and obliteration of the cervix, because the os cannot otherwise be opened. Free the uterine wall to the extent of at least half its circumference, so that the contents may be driven down through the expanded os, by the contractions of the free tissues.

To exemplify this, let me give in detail the following case:

On the evening of the 27th of September, 187-, I was requested to see Mrs. F., who was supposed to be threatened with premature labor. She was in the eighth month of gestation. She had recently suffered from one or two short but severe floodings. This evening bleeding returned, being severe and more persistent. On my arrival I found her free from bleeding or pain, though the loss of blood had been very considerable. Digital examination showed the cervix was not yet obliterated, but quite flaccid, the external os readily permitting the finger to pass through the canal to the internal os, which was open less than half an inch, and whose margin was very firm.

I believed that I could detect through that small orifice, the rough surface of a placenta. Placing the other hand on the abdomen of the patient, and pressing the foetus downward, I could plainly detect the bulky placenta between the presenting part of the child, and my fingers at the mouth of the womb. These manipulations did not produce any bleeding or pain, and I determined to "watch and wait."

The night was passed without return of the hemorrhage, and my patient awoke in the morning much refreshed, and feeling quite well. Enjoining strict quietude, and advising the summoning of a physician, should the least hemorrhage occur, I went my way with the sad reflection, that in these little words, *be still*, I had exhausted the resources of my profession.

Five days later I was again called to the bedside of my patient to find her almost pulseless; no pain—no bleeding—mouth

of the womb or internal os but little larger—margin not quite so wire-like.

Her first hemorrhage since I saw her last, was a very slight one, about twelve hours previous to my present arrival. The second bleeding had ceased a short time before my arrival. Examination did not bring on bleeding or pain, and I determined to give her time to recuperate, unless the hemorrhage should recur. Four hours later hemorrhage began. No appreciable change in the os—no pain, pulse barely perceptible at the wrist. Ordered ʒj. fld. ext. ergot given—passed the finger through the os, bored the point into the placenta, found it very dense. I then worked the finger around in every direction, detaching the placenta as rapidly as I could. Contractions came on immediately, but very weak—the bleeding ceased. I now flexed the finger in the os, and made traction in the direction of the pubic symphysis, and then again swept the finger around detaching the placenta as far as I could. After detaching the placenta for one inch or more in all directions, I restricted the process to one half of the circle, and continued it until I reached the margin. The ergot was repeated, the tension of the os kept up when the fingers were not otherwise engaged, the pains increased and became vigorous, the os dilated, the membranes were ruptured, the head presented, and delivery was effected in three hours from the time I began to detach the membranes. I am perfectly satisfied that there was not over half an ounce of blood lost, from the beginning of the manipulations. The child was well grown (large) and still-born. I believe it had perished from the loss of blood, before labor was induced. The mother seemed remarkably well, considering her extreme exhaustion, and continued so for about thirty-six hours; about this time she began to manifest an "irritative delirium," and died seventy hours after her delivery.

The placenta was seven inches in diameter, and the finger mark was exactly in the center.

This patient died from the loss of blood, a loss which preceded actual labor.

I believe the points in the above case are too plain to require pointing out to the reader, and that they will be seen fully to accord with the theory I have advanced.

Let this be the rule; induce labor in the above manner after the first serious hemorrhage, though it may have ceased spontaneously—or rather without the art of the physician. Do not trust to lemons, rags, sponges, styptics or nature—there is no safety except in delivery.

I know we are warned against interference after the hemorrhage has ceased, but I conscientiously believe that the only true and safe course to pursue, is that indicated above, and that when it shall have become the ruling principle in practice, "unavoidable hemorrhage" will be as completely under our control as post-partum hemorrhage.

Is not the rule here laid down, exactly the one followed by the surgeon in secondary hemorrhage? Who would refuse to ligate an artery involved in a secondary hemorrhage, because the bleeding ceased spontaneously, although he might have to open a half healed stump, or cut through sound tissues to find and tie the vessel? Who after amputation of the leg, would dress the stump without tying the posterior tibial artery, because it retracted, and refused to bleed? What obstetrician witnessing a hemorrhage on account of a retained placenta, would refuse to force its expulsion, because the bleeding ceased? Why do we insist on tying the artery, and on the expulsion of the placenta? Is it not to avoid the danger of a second hemorrhage? Then why not follow the same rule in placenta prævia? Truly we should.

I may be asked why not use styptics and tampons? Because experience and reason both, reject them as useless annoyances.

I know there are many seeming objections which may be raised against the views I have here advanced, but I believe they are only *seeming*, and will vanish at the approach of unbiassed investigation, like mists before the morning sun.

SELECTIONS.

The Transmissibility of Tuberculosis.

There is no question in modern pathology, human and comparative, fraught with greater interest and importance than this of the probable, nay, certain, transmission of tuberculosis from mankind to animals, and, possibly, from these to the human species. The solution of this problem is not only of urgent importance to sanitary science and the preventive medicine of the future, but so seriously concerns us individually of the present day as to demand our closest attention.

It becomes our imperative duty, then, to ascertain whether we are actually engrafting upon our children the fatal malady of tuberculosis by inadvertently administering to them milk derived from phthisical cows, as the experiments made by Klebs, Bollinger, and others lead us reluctantly to believe. It appears, also, that we are all exposed to nearly the same danger of infection by eating the flesh of cattle, when, as not unfrequently is the case, it is loaded with pernicious tubercular matter.

The revival, in certain quarters, of the belief that tuberculosis is contagious, even in the human species, as Morgagni asserted more than a century ago, suggests the reflection whether we, as physicians and teachers, lay sufficient stress upon the great danger of allowing healthy people, especially young children, to associate with those who are consumptive.

We have yet, it is true, a great deal to learn with regard to the pathology and communicability of this disease; but there can be no hesitation in recognizing the importance of the results already achieved by careful experimentation and observation. Startling as the above assertions may appear, that milk and flesh, such important articles of our food, as well as mere cohabitation, may prove media for the conveyance of tuberculosis, yet nevertheless, in the presence of so many important facts, we must yield them our credence. In short, we fully concur with Chauveau, who says, "It now appears proved that the identity of tuberculosis with the other virulent diseases is so complete and so absolute that we must either recognize its virulence, or deny the existence of virulence altogether. There is no middle place in this dilemma."

And now for our facts. But first let us see how the authorities agree as to the histological identity of human tubercle with that occurring in the lower animals. A remarkable unanimity of opinion exists upon this point at the present time. Professor Reynal,* of the Alfort Veterinary School, stands almost alone in maintaining that the bovine and human tuberculosis are essentially different in their histological features. While admitting that the elements of the bovine tubercle are incontestably identical with those of sarcomatous tumors, this writer, nevertheless, sees proper to separate it from the latter on account of the absence of nutritive vessels and the determinate volumes which can never be exceeded by the tubercular new growth. The majority of the authorities, however, appear to take the opposite view; of these, Prof. Schuppel,* who has made an almost exhaustive study of tubercle in general, and particularly that of the bovine species, has unquestionably fully established the absolute identity in structure and development of the cattle disease with tubercle in man. Accepting, then, the fact as fully established that human and bovine tuberculosis are identical, the numerous and apparently authentic and exact observations—a few of which will be given later on—as to the extension and transmissibility of tuberculosis in the different species become somewhat startling; but the results of experiments undertaken by various authorities with the view of ascertaining whether the disease could be produced by inoculation with or the ingestion of tuberculous matter, are yet more so. As most of these experiments are no doubt already familiar to many of our readers, we shall do no more than summarize the conclusions drawn from them, excepting a few of the more recent ones, to which, perhaps, as yet, less prominence has been given.

So prevalent was the tradition of the inoculability of tuberculosis that even Morgagni abstained from making autopsies upon the phthisical, and Laennec alleged that he had acquired a tuber-

* Reynal, "*Traité de la Police sanitaire des Animaux domestiques*," Paris. 1873, p. 701.

* "*Ueber die Identität der Tuberculose mit der Perlsucht*," Virchow's Archiv, Bd. 56, S. 38, 1872.

cular ulceration upon his finger in this manner; yet, strange as it may seem, and despite this long-noted tradition, it was reserved for Villemin to establish, in 1865, by actual experimentation, the certain transmissibility of this affection by inoculation. The animals experimented upon were chiefly rodents,—rabbits and guinea pigs,—and the material, tubercle from the human lung. The number of animals inoculated was large, and tuberculosis, more or less extensively developed, constantly resulted. No matter in what part of the body these inoculations were made, the tuberculization that followed was marked by the same serious characters in all, many of the creatures dying after a variable period, and others, falling into a state of marasmus, were killed. The lesions noted in them were chiefly found in the lungs, which were more or less infiltrated with tubercles; but other organs and textures were not exempted—the spleen, liver, bronchial and mesenteric glands, etc., being also involved in many cases. Experiments were also made upon ruminants and carnivora (sheep, dog, and cat), but the results with these were generally negative; though a few of them so inoculated were found with the characteristic gray granulations and cheesy deposits in the viscera. Villemin also determined by further experiments that the tuberculous matter of animals, especially that of the bovine species, could give rise to tuberculization. From these experiments he concluded that bovine phthisis is identical with that of man. The next step was to ascertain whether the tubercular matter thus produced by inoculation was also capable, like that which arises spontaneously, of being transmitted; or, in other words, whether its virulence was retained beyond the primary transmission. The second generation of tubercular matter was found to have retained all its morbid activity, causing intense tuberculization in an equally short period. Villemin concluded from his experiments that phthisis in mankind was the result of a specific or virulent agent introduced into the organism.

Since the first report made by Villemin, numerous other experimenters have tested the validity of his statements, so that the successful inoculation of the lower animals with the tubercular matter has ceased to be a novelty. Among these experimenters we find the names of Colin, Vulpian, Lebert, Cohnheim, Frankel,* Papillon, Nicol, Roustan, Laveran, Empix, Waldenberg, Wyss, Wilson Fox,† and Burdon-Sanderson; of these, the famous papers of the latter,‡ so extensively quoted by English writers, are probably the best known to American readers. All these experimenters succeeded in developing in rabbits and guinea-pigs a disease analogous to tuberculosis by inoculating them with tubercular products (gray granulations, caseous matter, and the sputa of phthisical subjects). It is evident "that the certain result of inoculation of

* Virchow's Archiv, Bd. xlix. S. 216, 1868.

† "On the Artificial Production of Tubercle in the Lower Animals," London, 1868.

‡ Sanderson on the Inoculability and Development of Tubercle, 10th and 11th Reports Med. Officer of the Privy Council, 1867-68.

tuberculous matter, of whatever kind, is to produce granulations in the lungs and other internal organs; that these granulations are anatomically identical with gray tubercle, that they become yellow or opaque, and eventually break down into a soft cheesy material. It is further admitted on all hands that the process is, so to speak, a self-destructive one; that in the act of inoculation a new element is introduced into the inoculated animal at the point of insertion, from which it is disseminated to all parts of the body through the lymphatics and arterial circulation."§ This latter statement is not invalidated in the least by the fact that the same effect may be brought about by the use of non-tubercular matter or the application of irritating setons, or other substances acting chiefly as mechanical irritants.

The results of these experiments seemed, it is true, to lead to views antagonistic to the idea of the specific nature and virulence of artificial tuberculosis; but they have been explained by stating that caseous deposits were first formed at the point of inoculation, or where the seton had been introduced, next in the neighboring lymphatic glands, and that it was the caseous matter thus formed which gave rise to the production of tubercles throughout the economy. This explanation is, moreover, entirely conformable to Niemeyer's theory of the development of tuberculosis in man,—a caseous deposit in any portion of the body (lungs, glands, articulations, bones, etc.) being, above all other things, according to this authority, a predisposing cause to tuberculosis; and if the lungs are so frequently tuberculous, this is because in them oftener than anywhere else this caseous matter is formed. He further pointed out that the infection of the blood does not always arise through a cheesy deposit, as alleged by Buhl, since cases of acute tuberculosis occur in man in which no trace of such a deposit exists.¶ He, with others, believed in a gradual generalization of tubercle through the medium of the lymphatics, while Lebert** thought that capillary embolism was always an important factor in the artificially-engendered tubercle. Buhl's theory seems, however, to have been generally accepted in so far as experimental tuberculosis in animals is concerned.

Villémin†† performed a further series of experiments to meet the objections brought against his views, and presents, in an address delivered before the Imperial Academy of Medicine in 1868, some very telling and convincing arguments with regard to the production of tuberculosis by inoculation. In these experiments he sought to realize, in principle, the conditions of a real inoculation,—to-wit, a very small wound and an inconsiderable quantity of inoculable matter. Although these conditions were never departed from, tuberculizations extremely variable in their intensity and generalization were obtained; every degree being

§ Sanderson, loc. cit.

¶ Niemeyer, "Pathologie und Therapie," Bd. i. S. 230, 7 Aufl.

** Virchow's Archiv, Bd. xli., xliii.

†† "Etudes sur la Tuberculose," Paris, 1868.

noted, from a few isolated granulations to those startling generalizations in which every viscus was crammed with the tubercular product, a manifest proof that *the intensity of tuberculization is completely independent of the quantity of matter inoculated*. Observing that the alterations in the lymphatic vessels and glands were far from constant, they often remaining unaltered, and the local tuberculization at the point inoculated was often quite rudimentary, while the viscera of the animal were richly tuberculosed, therefore, says Villemin, *the number and extent of the internal lesions have no relation to the local lesions at the seat of puncture*.

The transmission of tubercle has been likened to a process of grafting; "but how can such a process explain the myriads of tubercular granules which stud the parenchymatous and serous organs!" asks Villemin. "Grafted tissues continue to live and develop at the places where they are deposited, but they are not reproduced elsewhere in the organism. This theory, at the most, could only explain the development of tubercle at the seat of puncture; and even then the inoculated tuberculous matter, to do this, must be endowed with a very active vitality. But the softened matter in the center of a tubercle does not even contain any distinct elements; we inoculate only with a *detritus*. How can grafting explain the successful inoculation with sputa,—*sputa which have dried for twenty days*, as has occurred in our experiments? Does not all this prove that the inoculated matter acts by virtue of a principle independent of the histological elements entering into its composition?"

The *quasi*-tubercles induced in man and animals by various parasites, the microscopic appearances of which so closely resemble those of tuberculosis, are discussed by Villemin, as well as the results of injections of mercury, tallow, dust, and irritant substances of various kinds, even pus, into the bronchi and veins, and the great difference between them and true tuberculosis pointed out. To confound the former with the latter would be as great an error as to identify the pustules produced by tartarized antimony with those of variola.

Demet, Paraskev, and Zallonis, of Syra, Greece, not content with their success in transmitting this disease to rabbits by inoculation with the sputa and blood from a man affected with phthisis, performed the unprecedented and not-to-be-repeated experiment of inoculating a human patient whose history afforded no suspicion of a tuberculous taint, and whose lungs were perfectly healthy, but who was suffering from gangrene of the big toe of the left foot, due to femoral embolism. Amputation of the limb would not be submitted to, and, as a fatal termination was inevitable, a quantity of sputa from a man who had abscesses in his lungs was inoculated in the upper part of the left thigh. Three weeks afterwards, auscultation revealed very slightly increased respiratory murmur at the summit of the right lung, and somewhat prolonged expiration in the subclavicular region. Thirty-eight days after

the inoculation, the man died from gangrene; and, on examination of the body, it was found that the upper right lobe had seventeen tubercles in the first stage of development, of a gray color, and very hard. Two granulations each were found in the left apex and upon the surface of the liver. From this unusual experiment, as states Fleming,* from whose paper we have freely abstracted throughout, "it is evident that tubercle is inoculable in man himself, for it is scarcely possible that this patient, who was fifty-five years of age, could have had his organs infiltrated with as many as twenty tubercles in the first stage of development only. Had they been naturally developed, they must have multiplied and passed through their regular evolution, especially in an individual predisposed to phthisis. Their limited number, stage, and size, afford a direct relation to the brief interval separating inoculation from the decease of the patient." R. M. B.

(To be continued.)

Remarks on Esmarch's Bloodless Method.

Extracted from an address of H. B. SANDS, M. D., before the Medical Society of the County of New York.

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For the information of those who are not yet familiar with the details of the bloodless method, it will be desirable, first, to describe the mode and range of the application. Secondly, we may state its alleged advantages and disadvantages; and, finally, endeavor to estimate its value as tested by our own experience.

The bandages are composed of elastic webbing, or—what is better—of simple sheet-rubber, which can be more readily cleaned than the webbing, when it has become soiled by pus or other discharges. The bandages should be about two and a half inches wide, and it is convenient to be provided with several of them, each three yards in length, rather than to employ one long bandage. As a substitute for the tourniquet, some surgeons use a piece of the same bandage; while Esmarch himself recommends for this purpose a piece of tubing made of non-vulcanized rubber, which is softer and more yielding than the other varieties. The application of the bandage, in ordinary cases, is quite simple. It is put on with moderate compressing force, and carried from the fingers or the toes to any desired height. The constricting cord or tube, which is to take the place of the tourniquet, is then wound two or three times around the limb, immediately above the upper edge of the bandage, and made fast either by a knot, or by a hook and chain, or any similar contrivance. The bandage is then removed to expose the parts for operation. Some experience is needful to apply the bandage with the proper degree of force. Often it is applied too firmly, and this mistake is pretty sure to be

* George Fleming, "The Transmissibility of Tuberculosis," *British and Foreign Medico-Chirurgical Review*, No. cviii., October 1874.

made by beginners. On the other hand, it may be applied too loosely, and I have several times seen it put on with just sufficient force to allow venous congestion of the parts, with consequent hemorrhage during the operation. The same remark applies, with equal truth, to the use of the constricting cord, which, if applied too loosely, will cause venous engorgement, and, if too tightly, may do serious damage to the nerves, and probably also to the blood-vessels of the limb.

The skin, when exposed after the removal of the bandage, is seen to be blanched and anæmic; and, when the deeper tissues are cut into, they are found to be nearly or quite bloodless. The vessels, both arterial and venous, are almost absolutely empty, so that an amputation of the thigh, for example, can be performed without the loss of more than five or ten drops of blood. This bloodless condition remains unaltered so long as the constricting band is kept in position. Immediately after this has been removed, however—the large vessels having been previously secured—the blood returns to the parts with considerable force, and, if these are vascular, a pretty free oozing commonly takes place from the surface of the wound. This bleeding is usually quickly checked by the use of cold water, aided, if necessary, by gentle pressure.

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The advantages, as claimed by Esmarch, are as follows: First, the prevention of a great loss of blood during the larger operations. It is assumed that this economy of the vital fluid will sometimes be the direct means of saving life, that it will render more certain and speedy the healing process, and diminish the frequency of septicæmia and pyæmia. The other principal advantage of the method is the facility with which it enables the surgeon to accomplish deep and difficult dissections, the tissues not being obscured by blood. It is said also to permit the easy extraction of foreign bodies, and the thorough examination of diseased bones and joints, with the view of deciding upon the extent of operation necessary in any given case.

The following objections have been made to the bloodless method: It is alleged that the pressure impairs the vitality of the parts, and causes them, in many instances, to slough. Also, that it affects injuriously the nerve-trunks, giving rise to paralysis, both of sensation and of motion. Hemorrhage, occurring shortly after the operation, has been ascribed to the method. Its employment is also thought to be dangerous, because it may induce plethora of the internal organs, by driving back into the blood-vessels a larger amount of blood than they normally contain. It is feared that the vessels, if brittle from disease, might be ruptured from this cause. Lastly, it is believed that in cases of gangrene, or unhealthy suppuration, poisonous fluids may be pressed into the blood-vessels, or into the healthy tissues, by the elastic bandage, and thus occasion septicæmia or pyæmia.

In a second paper, lately written by Esmarch, that surgeon

gives the result of two hundred bloodless operations performed by him during the preceding year; and these results show the method in a most favorable light. As our own experience, however, is that on which we must rely, I proceed at once to a brief analysis of the cases which I have been able to obtain. They have been collected from the principal hospitals of New York, Brooklyn and Jersey City, and from the private practice of Drs. Wood, Krackowizer, Gouley, Ely, Weber, Little, Varick, and myself.

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On reviewing the above table, we find 63 amputations, excluding those of the fingers and toes. Of these operations, 44 were primary, for injury, and 19 secondary, or for disease. Of the primary amputations, 10 cases, or 22.7 per cent., terminated fatally—the causes of death being the following :

Pyæmia	4
Exhaustion or shock	3
Spreading gangrene	1
Erysipelas	1
Hemorrhage	1

Of the cases fatal from pyæmia, one patient had the disease at the time of the operation.

Of 3 cases of primary amputation of the thigh, 2 cases, or 25 per cent., ended fatally.

Of 11 cases of primary amputation of the leg, 3 cases, or 27.3 per cent. were fatal.

Secondary amputations, 19 cases, give a mortality of 21 per cent. The causes of death are stated as follows :

Tetanus (existing at the time of operation)	1
Exhaustion	1
Phthisis	1
Pyæmia	1
Total	4

Although I have not the means of making an exact comparison, I believe that the percentage of recoveries in these primary amputations, which were nearly all performed upon hospital patients, is somewhat larger than is commonly observed in such operations.

Out of 14 cases of excision of the joints, only one proved fatal; the cause of death being pyæmia. Another case will probably end fatally, however, as the patient was suffering from tetanus when last heard from.

* * * * *

No argument is needed to prove the excellence of the method, in enabling the surgeon to have a clear and unobstructed view of the tissues that come under his knife; and this feature of bloodless operations is, to my mind, the one of greatest value. Every one has remarked how much easier it is to expose an artery or a nerve

in the dead than in the living body. Now, this difference depends almost entirely upon the fact that, in the latter, the tissues are stained and altered in their appearance by the blood which escapes from the divided vessels. While the blood is still flowing, the parts are often completely concealed from view; but, even after the hemorrhage has ceased, the tissues are not unfrequently so uniformly stained as to make their recognition a difficult task. In these circumstances, especially if the dissection is a deep one, precision in operating is often impossible, and injuries, both unavoidable and serious, are not seldom inflicted upon blood-vessels and nerves, even by skillful and cautious operators. In a bloodless operation, however, these difficulties vanish, as if by magic, and dissection may be made upon the living body, with a degree of precision heretofore unknown. The aid thereby afforded to the surgeon is incalculable, and the advantage to the patient is great and obvious. To one who has never before witnessed a bloodless operation, that for necrosis of the femur in the popliteal space is, perhaps, one of the most striking. Instead of groping among tissues obscured by venous blood, and needlessly sacrificing the living bone, to permit the removal of the sequestrum, the operator can use his eye to guide the instrument at every step, distinguish at a glance the diseased from the healthy bone, and complete the operation with the least possible damage to the surrounding tissues. A medical friend, being present at such an operation, remarked to me that he had attended many operations for necrosis, but that this was the first one he had ever seen. In operations for caries of the joints too, particularly those of the hand and foot, the bloodless method permits the surgeon to inspect the diseased parts with ease and satisfaction, and to determine, to a nicety, the extent to which these require to be removed. In doubtful cases, we may thus often substitute for an amputation the minor operation of excision, or even an exploratory incision into the affected tissues.

* * * *

Its advantages were plainly shown in a case that occurred in my own private practice. The patient, a lady, whom I saw in consultation with Dr. Dana, had a sarcomatous tumor about as large as a walnut, growing from the periosteum of the radius, near the upper extremity of the bone. To remove it, I was obliged to turn aside the superficial muscles, and to cut away a part of the supinator brevis. In doing this, I exposed the posterior interosseous nerve to the extent of nearly an inch; but with care I dissected out the tumor without injury to the nerve. The wound healed in five days, by the first intention, and no paralysis followed. I feel confident that, had I not been able to avail myself of Esmarch's method, I should have probably either divided the nerve, or excised it. While pursuing this line of remark, I would suggest a caution. The bloodless state of the parts, while it favors the recognition of most of the textures, renders the blood-vessels themselves, in consequence of their emptiness, somewhat liable to

accidental division. Ordinarily, in operations such as are performed for the removal of deep-seated tumors, or for the ligation of vessels, both the arteries and the veins are easily recognized, the former, if at all large, exhibiting pulsation, and the latter revealing themselves by their size, color and compressibility. But, when the bloodless method is employed, I have found that considerable care is required to avoid the unnecessary division of vessels—the veins, from the thinness of their coats, being especially liable to this accident. During the operation, therefore, the surgeon should make good use of his anatomical knowledge, and study the appearance of the tissues before he divides them. Otherwise, what is a bloodless operation in the beginning, may in the end be a very bloody one; and the operator may be chagrined to find, when the pressure of the cord is removed, that he has unwittingly severed one or more important vessels.

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Having considered the advantages of Esmarch's method, as shown by the facts which I have collected, let us now try to determine whether they prove it to have any disadvantages. What I have to say may be put under three heads, namely, sloughing, secondary hemorrhage, and paralysis.

Sloughing is reported to have followed in eight of the operations recorded in the table.

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On reviewing these cases, we find that seven of them were cases of primary amputation. As the whole number of primary amputations given in the table is forty-one, it follows that sloughing of the flaps occurred in seventeen per cent. So far as these figures are concerned, they prove nothing against the bloodless method; for it has been shown, by Dr. Figuera, that, at Bellevue Hospital, out of forty amputations, according to the old method, the flaps sloughed more or less in twenty-four, or sixty per cent.

Ten cases of sloughing are reported after secondary operations, these being nearly twelve per cent. of the entire number given in the table.

After a careful examination of the cases in which sloughing followed in primary amputations, I hardly feel willing to attribute the accident to the method employed. When we consider that some of these patients were intemperate, or feeble from loss of blood, that their injuries were severe, and that the flaps, which, in some cases, were very long, were generally taken from parts not far from the line of laceration, we are strongly inclined to regard these circumstances as the most important factors in the production of gangrene.

* * * * *

It is greatly to be hoped, however, that by a careful application of the elastic bandage, we shall be able to secure the benefits which it unquestionably confers, without any compensating disadvantages; meanwhile it will be prudent to abstain from its

employment in certain cases, and, above all, to learn the minimum degree of pressure that will accomplish the desired result. The bandage should be soft and highly elastic, and the constriction of the limb should be made either by a piece of the same material, or, where this would be too wide, by a piece of soft rubber tube. The solid cord should, I think, be abandoned, as likely to do mischief. In applying the constricting band, much less force is needed than is commonly imagined, and with every additional turn the effect is considerably increased. Finally, the operation ought not to be needlessly prolonged.

* * * *

I have been unable to discover in the cases of operation that were not followed by sloughing, any failure or imperfection in the reparative process, which could be ascribed to the method employed. The wounds have presented the ordinary character, and many of them have healed by the first intention.

Hemorrhage is reported in four cases in the table.

In none of these cases, so far as I am able to judge, was the bleeding fairly attributable to the employment of Esmarch's method, and I am therefore disposed to dismiss, as invalid, the assertion that the method predisposes to the occurrence of hemorrhage.

We find in the table only one case of paralysis; but this was well-marked, and occurred on the day following an operation, performed at the Roosevelt Hospital, for necrosis of the humerus.

Enough has been said to prove that Esmarch's bloodless method is one of the most valuable surgical expedients that have been devised in modern times. It only remains, now, to determine the best mode, and the proper range, of its application.—*N. Y. Med. Journal.*

Therapeutics of Malarial Fever.

By PROF. W. HERTZ, of Amsterdam.

[Translated for Vol. II. American Edition of Ziemssen's Cyclopædia of Medicine, by E. W. Schauffler, M. D.]

Malarial Fever is one of the diseases in which *prophylactic measures* are of the greatest importance, belonging in part to the domain of public hygiene and in part falling within the scope of individual effort. Swamps, in malarial regions, should, if possible, be laid dry, or when this cannot be done, means should be used to keep their surface constantly submerged. Ditches of stagnant water should not be allowed in the vicinity of towns.

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The streets of cities should be paved and kept clean. Furthermore it is the duty of those having charge of the public health to provide a plentiful supply of pure drinking water.

[Here follows a series of instructions to those about to visit malarial regions as to the season of the year most desirable for selection, together with precautions as to clothing, diet, exposure to night air, and habits of life generally.]

The prophylactic action of Quinine, which was first observed by Bryson in the fevers prevalent on the coast of Africa, has, of late, received ample confirmation on the part of various writers (Balfour, Baikie, Saussure, and Rogers). The dose used was $2\frac{1}{2}$ to 3 or $4\frac{1}{2}$ grains, two or three times a day. Jilek* reports, from Pola, that, in a garrison of 736 men, 500 men were given daily, at first 3 grains, afterwards $1\frac{1}{2}$ grains of quinine apiece, and a pint of wine, while the remaining 236 men were left entirely without treatment. Ninety-one of the first lot (18 per cent.) were taken sick, though in a mild degree and without many relapses; of the last lot, or those without treatment, 68 (28 per cent.) were attacked. Subsequent experiments at the same place by Boxa did not yield as good results, and he recommends, instead of the daily smaller doses, the giving of a larger dose (12 grains) once a week. Colin's experience in Italy and Algiers also failed to confirm the favorable influence of this treatment, especially in preventing relapses, and he believes that the system becomes accustomed to the remedy and so it loses its effect entirely.

My own observation on this point, made both on myself and others, leaves no doubt as to the efficacy of this daily preventive dose. In some cases the attacks are entirely prevented during this treatment, in others they only appear in the lightest forms. I have never seen any protection afforded against relapses, and I have noticed that a tolerance of the drug was established, so that, in order again to obtain its full effect, it became necessary occasionally to omit the treatment for two or three weeks at a time.

Those who live where malarial disease is endemic will do well to pay attention to the first signs of being sick, and a dose of 9 to 12 grains of quinine on going to bed at night, for three or four nights in succession, is usually enough to break up an intermittent attack. I have never seen any special results from the use of the tincture of gentian or of nux vomica, which are also recommended as prophylactics.

The *febrile attack itself*, if it is a simple intermittent, usually demands no treatment, in our part of the world. All that is required is to guard the patient against injurious influences, and to this end, even in the milder cases, it is well for him to take to his bed on the first appearance of a paroxysm, and abstain from all food until it is over, for fear of inducing vomiting. During the *chill*, it is best not to pile on too heavy bed-clothing, which only fatigues, nor to give very copious hot drinks. The chilliness is not removed by such means, and some of those recommended for this purpose, as hot or steam baths, alcoholic drinks,

*Wochenblatt der Gesellschaft der Wiener Aertzte 1870, No. 17.

running, or other severe bodily exercise, cannot but do harm. Even in severe and long-continued chills we must content ourselves with friction of the skin, warming of the bed, and if thirst is excessive, with the administration of a moderate amount of warm tea. In persistent vomiting, effervescing mixtures, or small doses of opium are indicated. Where, as is most likely to occur in children and aged persons, the vital powers sink, the pulse becomes small and irregular, the respiration shallow, and collapse is threatened, the analeptics may be resorted to boldly; such as strong wine, ether, camphor, and strong, black coffee internally, inhalations of ammonia, sinapisms and friction with spirits and mustard externally, or stimulating enemata.

During the *hot stage*, cool surroundings and light covering are desirable, a moderate indulgence in cool, refreshing drinks, and, in case of vomiting, broken ice by the stomach. If severe headache and delirium are present, giving evidence of considerable congestion of the brain, cold water, or ice compresses should be applied to the head, and the body be sponged with vinegar and water; vinegar clysters may also be given.

The *sweating stage* demands no special attention, and no steps need be taken to encourage diaphoresis. If the intermission proves to be incomplete, it will be safer to keep the patient in bed.

In *pernicious* intermittent fever, the expectant plan, which is appropriate during the attack in the simple form, is entirely inadmissible. Here alarming symptoms crowd fast on one another, and it is often only by prompt and vigorous interference, on the part of the physician, that life can be saved. Nor is it only the alarming symptoms that occur during the attack, but the disease itself, that must be combatted by specific means whose value has been thoroughly tested.

Where the fever has a more sthenic character, and especially in the pernicious icteric intermittent, it has often proved of advantage to precede or accompany the specific quinine treatment with vigorous purgation. The best article to use for this purpose is Calomel, in doses of from 4 to 8 grains, given repeatedly, as required. In the delirious form, we should still avoid venesection, even in robust persons. If the abstraction of blood is demanded, it should be done locally; usually, however, cold water or ice compresses to the head, derivation by enemata, and counter-irritation to the extremities, are to be preferred. In coma and apparent death, stimulating enemata, strong irritants to the skin, and tincture of musk subcutaneously are indicated. In violent vomiting, opium, effervescing draughts, ice-pills, camphor, and strong irritants over the stomach. In severe chills, brisk friction and hot bottles or bricks; in the algid form, rubbing the body with camphor and mustard spirits, or with ice, and internally stimulants, such as ether, champagne or strong wine. In all cerebral symptoms which are accompanied by great excitement or restlessness, opium or morphine should be given, and it is best to

combine it with quinine. In the diaphoretic fevers, mineral acids, given in dilute form as a drink, seem to check the excessive sweating.

Quinine still occupies the front rank among the remedies known, empirically, to cure malarial fever. The former practice of prescribing it in frequent small doses, two grains every two hours, which is still sometimes met with, ought to be abandoned, for malarial fever; because, on the one hand, the entire intermission is often occupied in this way without the patient's receiving enough of the drug, and, on the other, the doses given shortly before the paroxysm have no time to act, and an absolutely larger amount is required to prevent an attack, if thus given.

I have often seen patients who had been treated in this way for months, without effect, and in whom a few large doses broke up the fever.

In simple intermittent, 9, 12, 15, or 22 grains should be given as a single dose during the intermission; or $7\frac{1}{2}$ to 12 grains twice to three times, but in such a manner that the last dose shall be taken five or six hours before the next attack is due. In younger persons, from sixteen to twenty years of age, one-third less may be given; children from ten to fifteen years old, should have one dose of 7 grains, or two doses of $4\frac{1}{2}$ grains; children from three to six years old, one dose of $4\frac{1}{2}$ or two doses of $1\frac{1}{2}$ grains. These amounts are calculated completely to subdue the disease, if it is simple intermittent fever, especially in recent cases. Often, however, particularly in those that are not very recent, a second, or perhaps even a third attack may occur, although much weaker, and, as a rule, postponed. If relapses take place, one dose is usually enough for their removal; where, however, they are frequently repeated, and especially where the constitution of the patient is being undermined, it is necessary to employ the remedy for a longer period, usually in combination with iron. To adults I usually give $4\frac{1}{2}$ to 6 grains, morning and night; to children $2\frac{1}{4}$ grains. It is desirable, for the sake of preventing relapses among those who reside in a malarial region, to take a dose of quinine for a while on those days when the paroxysm would be likely to return, if at all; for instance in a tertian intermittent, on the 3d, 5th and 7th days, etc. It is a mistake to suppose that the paroxysms always recur on the 7th, 14th, 21st or 28th days, and that it is only necessary to guard against them then.

In pernicious malarial fever it is usually necessary, on account of the more imminent danger, to increase the dose considerably, and, as has already been stated, not to wait for an intermission, but to give the medicine during the attack. I give 30 to 45 grains a day, usually in two or three 15 grain doses. Maillot once gave 180 grains during 24 hours, with a good result. The use of quinine must here be continued until the alarming symptoms have disappeared. When large doses, given in solution,

are vomited, or where there is extreme irritability of the stomach. the tincture or extract of opium and aromatics may be added. If these, also, are rejected, or if the patient cannot swallow, the drug must be given per rectum. In that case the dose is 3, 7½, 9 or 15 grains, and in pernicious fevers as high as 30 grains, combined with opium.

The hypodermic use of quinine is also practiced, under the same circumstances, and has the advantage of more rapid action as well as a smaller expenditure of the agent. From one-fourth to one-third of the ordinary dose is sufficient when injected under the skin.

Aside from its value as a prophylactic, quinine shows its power the most in controlling simple intermittent fever of the quotidian and tertian type. It is less effective in quartan intermittent, and in pernicious fevers with grave local manifestations, and least so, of all, in the remittent and continued forms. Nevertheless, it is unquestionably true, that, even in the latter, it is the most effective remedy that we possess.

[A continuation of this paper, treating of the other preparations of Cinchona, of Eucalyptus, Arsenic, etc., and of the treatment of some unusual forms of malarial disease, will be given in our next, if the 2d Vol. of the Cyclopaedia does not appear before that time.]

Destruction of One Lung by Inflammatory Phthisis.

BY PROFESSOR ALONZO CLARK, M. D., NEW YORK.

* * * * *

It is common enough for people to say of themselves, or of another, "One lung was (or is) all gone;" but an examination contradicts this statement, with so much certainty, that I, for one, had about persuaded myself that death must occur before so much devastation could be accomplished. But here is really a case of phthisical or inflammatory destruction of one lung.

U. P. Cone, a little past twenty-three years of age, of good habits, rather short in stature, but otherwise well developed, having the appearance of good health, on the first Sunday of April, 1871, was engaged most of the day as secretary of a very large Sunday-school, in attending public worship and a Bible-class, and went to bed in usual health at ten o'clock. At half-past twelve he was awakened by an hæmoptysis. The hemorrhage was considerable, though not really profuse. He did not remember to have had cough, but on the 7th of January previous had some hæmoptysis while traveling. The father is a healthy man, but has lost one sister of phthisis, and his paternal grandmother had "old fashioned consumption" (*i. e.* very chronic). On the mother's side no instance of this disease could be cited.

The hemorrhage was not then repeated; but for the next eight days the patient kept himself very quiet and free from ex-

citement, when he was attacked by pneumonia of the left side, beginning with a chill, attended with pain in the side, and soon with chocolate-colored or "prune-juice" expectoration. This latter was unusually profuse, amounting at one time to about half a pint in twenty-four hours; but it gradually ceased, lasting about a week in all. During this attack considerable blood was raised with the pneumonia expectoration.

I saw him for the first time on the 15th of April. This was the fourth day of the pneumonia; he was still coughing up blood pretty freely. The question was raised, whether this pneumonia was provoked by bloody infarction from the late hemorrhage.

April 16th. The pneumonia had solidified nearly the whole of the left lung. The pneumonic fever was high and exhausting; this and the pain in the side were considerably reduced by applying cloths wrung out of hot water to the whole anterior of the chest and abdomen, (fomentations). These and hot foot-baths relieved the pain and produced sleep.

April 20th. The rational symptoms of pneumonia, such as pain in the side and head, and the continued fever had abated; but the pulse was rapid, the pneumonic consolidation was not diminished, and rales were becoming audible over much of the lung.

May 4th. The whole left lung still solid; afternoon fever and night-sweats, severe pain renewed in left side.

May 17th. Expectorating freely, hectic continues, loss of appetite, emaciating rapidly, inflammatory and phthisical consolidation not diminished, rales everywhere in left lung.

June 16th. Patient confined to his bed for a little over two months. No material change of symptoms; sweats, emaciation, and consolidation as before. Although too feeble to walk, Dr. C. will to-day take his son to the country, (Green Co., N. Y.).

The effects of this change were very marked. In the country the patient soon regained his appetite, the sweats ceased, he gained strength and flesh. He improved slowly but steadily for four months, having, however, all the time, cough, and more or less of expectoration.

Nov. 18th. Returned from the country, still pale and thin, has strength to walk about town for an hour at a time; no hectic or hemorrhage, but a large cavity in the upper part of left lung, rales and dullness in all the rest of it. "Worse than would appear from the general symptoms," is a part of my note at the time.

From this time onward I saw him at long intervals, and did not record his condition again till the time of his death. In general, with the aid of his father, I can say that his health appeared to improve; that the left side of chest became more and more contracted, and the spinal column, curved in the thorax from the age of fourteen to the left, became more curved. He always had cough, more or less, with expectoration; but for eleven months

before his death he was able to give the usual time and attention to a business which he was conducting successfully, with the aid of a partner, up to the day of his fatal seizure, nine days before his death.

In this last attack he had three bleedings; one on Friday, January 24th, 1874, pretty abundant; another on the Tuesday following, more profuse than the first. Twenty minutes after this bleeding ceased, he vomited a considerable quantity of blood.

On Sunday morning the bleeding returned again, and was rapid, profuse and fatal.

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A *post-mortem* examination was made on the third day after death.

* * * * *

The thorax was markedly depressed and contracted on the left side. Spinal column curved markedly to the left, the highest point of convexity being at the seventh and eighth ribs. The capacity of this half of the chest was greatly diminished.

On opening the thorax the lung tissue was found occupying the left as well as the right cavity, but it was soon ascertained that the right lung, while it filled the right cavity, extended to the left, partly by emphysematous expansion, so that it appeared to fill the diminished left cavity. Turning back the extended portion of the right lung, nothing was found in the left chest but the heart, and a hard mass, three inches by two and a half, lying against the side of the spinal column, between the second and fourth ribs, firmly adherent to the posterior of the thoracic wall. In this there were no traces of the vesicular tissue of the lung, nothing like air-cells, but instead a little cheesy matter and melanotic coloring material, of the latter not a large quantity. A bronchus of large size, perhaps enlarged (bronchiectasis, Cowle), opened out of this mass, but the mass could not be inflated through it. A few remains of the smaller bronchial tubes were seen in the middle of the mass, and it was covered by a continuous smooth membrane, which appeared to be the thickened pulmonary pleura, without perforation or cicatrix. Some fibrous tissue was seen amidst and around the cheesy matter and under the investing membrane (pleura). The melanotic material may have been derived from the bronchial glands, which were indistinct and partly blended with the other constituents of the mass. The lining membrane of the remaining bronchial tubes was only slightly reddened. The tubes contained no blood, and no blood-vessel was discovered which seemed to have been the source of the fatal bleeding. Dr. Cowle, however, noticed, as he thought, an ulcerated point at the outer end of the large bronchus, and thinks that the blood was discharged from that point.

The left pleuritic cavity contained no air or gas, no serous fluid, no pus.

The pleuritic membrane of this side in the inferior portion of

the cavity was dotted with tuberculous granulations. Otherwise the costal and diaphragmatical pleura does not appear to be unnatural.

The heart had fallen back into the posterior curve of the ribs, and the pericardium was attached by easily broken, fibrous bands to the posterior wall of the chest, and it had fallen downward a little, so that its apex was on the level of the sixth left rib. Consequently this poor, unbreathing residuum of the left lung was almost wholly above the heart, while both had contracted adhesions, more or less firm, to the posterior thoracic wall.

The right lung, by this retreat of the heart and great vessels, was permitted to pass over in front of them into the left cavity. This implies yielding or absorption of the mediastinal membranes. Their condition was not noticed at the autopsy.

It was chiefly by the expansion of the upper and lower lobes that the left cavity was filled. "The middle lobe," Dr. S. says, "rested on the vertebral column." Besides this great enlargement, the upper and middle lobes were studded with bright gray miliary tubercles; a few only in the lower. The upper and lower lobes were emphysematous, and the lower was congested with blood. There was no cavity, and no evidences of hemorrhage in any part of this lung.

* * * * *

Here, then is a case that will bear a close examination.

1. The function of one lung was completely abolished, and yet the subject of this destruction had recovered from a dangerous sickness while the destructive process was going on, and enjoyed what was called good health after it was completed.

2. This destruction was the consequence of a pneumonia, which in the first week was only distinguishable from the most common form of that disease by the facts that it occurred a week or eight days after a pulmonary hemorrhage, and that it was attended by a free expectoration of blood.

3. This pneumonia, however, did not resolve—never was resolved by either of the usual methods, vesicular softening and expectoration, or vesicular softening and absorption—but after six or eight weeks began to break down, with a sort of ulcerative action that destroyed all the different lung tissues at the same time; and seven months from the beginning of the pneumonia this process had excavated all the upper portion of the lung, leaving the middle and inferior portions, so far as can be judged, to the five or six following months.

4. The proper pneumonic symptoms were followed, after an interval of about two weeks, by symptoms of phthisis, which became extreme in two or three weeks from their commencement, having, however, a duration of only a few weeks, with the exception of cough and expectoration.

5. Were the tubercles found after death, the offspring of "cheesy degeneration" of the left lung, or do they belong to the

diathesis which caused the hæmoptysis that preceded the pneumonia?

6. It would hardly seem possible that the pneumonia and after-obliteration of the lung should not have obstructed the circulation from the right heart, and set back the venous blood upon the large viscera of the abdomen. Yet there was no hypertrophy of the right ventricle; and while there was infraction to a limited extent in the left lobe of the liver, and enlargement of the spleen, the liver is described as smaller than natural. The size of the pulmonary arteries, right and left, is not referred to; but it is fair, under the circumstances, to infer that the right branch was materially enlarged, and the right heart in this manner relieved.

7. In rapid and overwhelming hæmoptysis it is common to find that portions of the blood have been drawn into the lung, producing spots of pulmonary apoplexy. In this case nothing of the sort occurred, but the blood not actually discharged from the mouth seems to have found its way into the stomach.—*N. Y. Medical Record.*

The Neurotic Origin of Disease and the Action of Remedies on the Nervous System.

The following extracts are made from a synopsis published in the *Phila. Med. Times*, of Dr. F. D. Lente's paper, read at a recent meeting of the New York Neurological Society:

He also instanced articular rheumatism as an example of a neurosis. Dr. J. R. Mitchell attempts to prove it a neurosis, and refers its phenomena to a spinal origin. In a case of pruritus reported in the first number of the *Archives of Dermatology*, by Dr. Beard, for Dr. Kinsman, of Ohio, the latter remarks, "the moment the current (of electricity) is passed along the spine the pruritus ceases." Dr. Beard also reports a case of herpes zoster frontalis for Dr. Bulkley, cured by galvanism. The spinal origin of many cutaneous affections is well established. It is important, in all cases of very obstinate eczematous or bullar diseases, to pay special attention to the influence of the nerve-centres and of possible nerve-lesions as a cause.

The peculiar effects of cathartics, especially where they are given for their so-called derivative action, can only be explained by this reflex influence on the brain and its vaso-motor vessels rather than by unloading the abdominal vessels, since the impression felt in the brain is almost instantaneous. This is felt sometimes even after an ordinary evacuation, or even after the passage of gas.

* * * * *

In regard to the action of the bromides, there is one rather wide-spread error which tends greatly to circumscribe their usefulness, namely, that in cases of cerebral anæmia, or in a condi-

tion of the circulation in which it may be supposed that this exists, they are useless, and even injurious. Now, the result of experiments with these drugs on animals, carefully collected, as well as those of clinical experience, go towards demonstrating the fact that the least important of the effects of these agents is their vaso-motor influence; that their most important action is in controlling and, if pushed far enough, almost obliterating reflex action,—first, by a direct effect on the nerve-centres, the medulla oblongata, and the cord especially; secondly, on the peripheral nerves: that is, blunting the susceptibility of the latter to painful impressions, and also the susceptibility of the cord as to those impressions. This important virtue of the bromides is well exhibited in preventing sea-sickness and the vomiting induced by anæsthetics, and the unpleasant effects of opium and quinine, which clinical experience has recently taught us. This property has special applicability in the treatment of certain functional diseases of the heart.

As regards the use of the active neurotics, it is certain that most of the failures and disappointments often complained of have resulted from inefficient doses. We may give this as a rule of action, that the drug cannot be said to have had a fair trial unless it has relieved the symptoms or produced its toxic effects. With regard to the bromides, strychnia, opium, atropia, physostigma, this is especially the case; and in very dangerous cases the toxic effects need often to be very pronounced. The doses, in fact, are as various as the temperaments and idiosyncrasies of the patients. For an account of the employment of large doses of physostigma, reference was made to the last number of the *London Practitioner*. The patient, a physician, took, in four days, of the solid extract of a reliable manufacturer, sixteen, forty-eight, fifty-seven, and seventy-two grains at one time; serious paralysis was induced, but the urgency of the disease, tetanus, required a perseverance with the drug.

A METHOD OF INDUCING PREMATURE LABOR.—The plan which I here pursue I have found upon a long and faithful trial to give better results than any other. This is nothing more than the practice of the original method, consisting in the employment of a pointed quill, or, what will answer the same purpose, a steel pen. This method, besides having the advantage of simplicity, is always applicable, as the instrument is ever at hand. The point of the quill is placed upon the palmar surface of the index-finger, which is then passed up to or through the cervix, and the membrane punctured. An improvement on this plan consists in making an opening in the side of the quill through which a sound can be introduced. The point of the quill is then brought into close apposition with the body of the sound. The latter is then passed through the cervix, the quill being kept in

position, and when the desired distance is reached the sound is withdrawn, leaving the quill behind, and the puncture can be made. In this manner we overcome the difficulty of passing the projecting angle of the posterior wall of the cervix.

By this method no damage can result, and a long narrow cervix can be readily passed. As the amniotic fluid drains away, pains are induced, the head passes down, and in twelve hours delivery can generally be accomplished.—*Braun : Phil. Times.*

A Case of Poisoning by Strychnia, Successfully Treated with Chloroform and Cannabis Indica.

By LAWRENCE JOHNSON, M. D., New York.

At about 8 P. M., September 29th, 1874, A. A. R., a healthy young man, aged twenty-two, with suicidal intent, swallowed five or six grains of strychnia in a glass of soda-water. He had obtained it of a druggist upon the pretence of wishing to poison rats; and the druggist informed us of the quantity furnished.

I was called to see him about fifteen or twenty minutes later; found him half-reclining upon a chair, being held in that position by a man on either side. He was in violent tetanic convulsions, every muscle seeming to participate, his face livid, and frothy saliva flowing from the mouth. I immediately sent for chloroform, which was procured in about five minutes, and having laid the patient upon the floor, proceeded at once to administer it to him by inhalation. He passed under the anæsthetic influence rapidly, and simultaneously his convulsions ceased.

About this time Drs. Wm. C. McFarland and T. L. Janeway arrived, and we consulted together. We concluded to continue the chloroform, allowing a brief respite now and then to note the effect of its suspension.

During one of these intervals shortly afterward, our patient opened his eyes and spoke, asking for drink, etc. Giving him water, he drank it, but with difficulty, the exertion bringing on a convulsion. With the idea of getting rid of any of the poison remaining in the stomach, we gave him two doses of sulphate of zinc, grs. xv. each, at within about ten minutes of each other. Shortly afterward he vomited freely, ejecting a large quantity of liquid, together with some undigested food. During the whole of this time every effort at swallowing was accompanied or followed by convulsions, as was nearly every voluntary movement, or even a current of air blowing over him, and consequently he was kept steadily under the influence of chloroform to a greater or less extent. At 10 P. M. the convulsions continuing whenever chloroform was suspended, we gave \mathfrak{z} i tr. cannabis indica, and ten minutes later another dose of like quantity. Soon afterward he vomited. Half an hour later another dose of \mathfrak{z} i was given, and two more at intervals of half an hour and one hour, all of which were retained. The convulsions meanwhile diminished in frequency and

force, and chloroform was only resorted to when they occurred. After 11 o'clock convulsions seldom occurred spontaneously, for the most part being directly induced by voluntary movements upon his part or by his attendants accidentally touching him. At 1 A. M. we considered him out of danger and left him for the night.

He had but one convulsion after our departure, at 3 A. M., and at 9 A. M. we found him able to walk to his home, half a block distant.

How much of our success should be attributed to chloroform and how much to cannabis indica is of course a matter of conjecture. That sufficient strychnia had been absorbed to produce death I think the violence of the initiatory symptoms warrant us in assuming. The chloroform in the first instance assuredly saved life. Yet inasmuch as there seemed no permanent improvement until cannabis indica was administered, while after that time the gain was rapid, I think the conclusion that its action was that of an antidote a very reasonable one. To us who watched this case very carefully the conviction is as strong as could be made by a solitary example.—*N. Y. Med. Record.*

On the Artificial Nourishment of Infants Suffering from Acute Intestinal Catarrh.

By DR. R. DEMME.

Translated for the Kansas City Medical Journal.

As the result of twelve years' experience with bottle-fed infants, in the Children's Hospital at Berne, the writer recommends the following as the best nourishment in cases of gastric or intestinal catarrh: Let $\frac{1}{4}$ of a pound to one pound of lean beef, finely cut, be soaked from half an hour to an hour in four pints of cold water; then boil it down to one pint; on growing cool, let the fatty scum be removed and the preparation filtered and set aside. Whenever it is to be used, say every two or three hours, this beef tea, *without having been previously warmed*, is to be mixed with freshly prepared rice-water or barley-water. During the intervals of feeding, the latter articles may be given alone, to quench thirst, it being preferable not to sweeten them.

If the children refuse this diet, they may be given Albumen-water, prepared by merely mixing the whites of from one to three eggs with half a pint to a pint of water that has previously been boiled;—this being given without any flavoring or thickening. Should the children become seriously reduced in strength, it will be found necessary to add 5, 20 or 30 drops of the best Brandy to the rice, barley or albumen-water from three to five times a day.

In the case of older children it may be safe to try milk mixed with mucilaginous soups.—*Allg. Med. Cent. Zeitung.*

Angina Pectoris.

In one of his lectures, M. See treated of angina pectoris—a disease well known, and, in general, easily recognized by medical men. Symptomatically and nosologically, he stated, the disease was perfectly defined, but its pathology was still involved in obscurity. Various theories have been propounded as to its pathology, which may be summed up as follows: The theory known as the English theory, which is to the effect that angina pectoris is always the result of a lesion of the heart or large vessels; the old German theory, which attributes it to the gouty diathesis; and the French theory, that it consists of a painful neurosis or neuralgia of the cardiac plexus of nerves, which neuralgia is sometimes idiopathic or spontaneous, sometimes symptomatic. M. See does not admit the existence of angina pectoris without a lesion, and would, therefore, be more favorably disposed to accept the English theory. The following case, which has presented itself in his ward, would seem to justify his preference for the English theory. A patient was admitted in October last with marked symptoms of anxiety and dyspnoea; the latter did not resemble the dyspnoea of asthma, nor that dependant on disease of the heart; notwithstanding the distressed state of the patient from the dyspnoea, the rhythm of the respiration was regular, without any appreciable disturbance in the act of inspiration or of expiration. The patient complained of the two symptoms characteristic of the disease, which in fact may be considered pathognomonic, particularly the second; viz., pain in the præcordial region and pain in the left shoulder. On auscultation, a *bruit de souffle* was heard at the base of the heart, and also, though slightly, at its apex. The paroxysms occurred at first at lengthened intervals; the intervals became gradually shorter, until at length he had five or six attacks a day, and finally the pain was almost constant. In the intervals of the paroxysms, there was not the slightest dyspnoea. The patient was free from dropsy or any sign of pulmonary congestion. He had nothing in fact that would lead one to suspect disease of the heart. Feeling somewhat relieved by his stay in hospital, he asked permission to leave; but he was not long out when he applied for readmission, and in three days after he was carried off in one of the paroxysms. The following is a summary of what was found at the necropsy: The heart was dilated and hypertrophied, particularly the left ventricle. The aorta was arteromatous, nearly cartilaginous; the atheroma caused a considerable swelling above the sigmoid valves. In consequence of this swelling, the orifice of the coronoid artery was extremely hard and contracted, it had lost its elasticity, and its calibre was reduced to nearly half its normal size. One of the principal columnæ carneæ of the heart was hard and diminished in thickness, which, with the dilatation of the heart, contributed to produce comparative insufficiency of the mitral orifice. Finally the coronary artery was contracted throughout its length, and was

even completely obliterated at a certain point of its course. This would explain the *bruits de souffle* which were heard during life. It is principally by the existence of this lesion that the mode of death in this case may be explained. The coronary artery not being able to convey a sufficient quantity of blood to the heart, the condition of the latter being in consequence considerably modified, it ceased suddenly to act; hence fatal syncope. The nervous system of the heart was not examined. As it is, the post-mortem examination, though incomplete, is full of interest, and the case is worthy of more profound study, which M. See intends prosecuting with the aid of his able *chef de laboratoire* Dr. Cornil.—*Brit. Med. Journ.*

THE PHYSIOLOGY OF VOMITING AND THE ACTION OF DRUGS UPON IT.—Dr. T. Lauder Brunton, the present editor of *The Practitioner*, in a highly interesting article on the above subject, submits the following conclusions:

(1.) Vomiting consists in two factors, viz: (1) the simultaneous compression of the stomach by the abdominal muscles and diaphragm; and (2) the opening of the cardiac orifice by the contraction of the longitudinal fibres of the œsophagus.

(2) When innervation is disturbed, these two factors do not occur together, and thus retching may occur without vomiting.

(3) The movements of vomiting are correlated by a nervous centre in the medulla oblongata, from which impulses are sent down through various motor nerves to the muscular structures engaged in the act.

(4.) This nervous centre is probably closely connected with the respiratory centre, but is not identical with it.

(5.) It is usually set in action reflexly by irritation of the pharyngeal, gastric, hepatic, enteric, renal, uterine, ovarian, and possibly also by the pulmonary and vesical nerves which come from the periphery towards it. It may also be excited by impressions sent down to it from the brain.

(6.) Vomiting may be arrested in two ways, either by removing the irritant which is exciting the vomiting centre, or by lessening the excitability itself, so that the centre no longer responds to the impressions made on it from without.

(7.) Emetics may be divided into two classes: those which act only on the stomach, and those which act on the vomiting centre also.

(8.) Tartar emetic probably acts in both ways. Tolerance of it is probably due to want of hydrochloric acid in the stomach.

(9.) Emetics may be used to evacuate the stomach and duodenum. They thus remove irritating matters, poisons generated in the stomach by putrefaction, bile, and metals or fever poisons circulating in the entero-hepatic circulation.

(10.) They may also be used to empty the bronchi and gall-bladder, or to cut short epileptic and to prevent ague fits.

INFLUENCE OF NUTRITIVE CHANGES AND EXTERNAL CIRCUMSTANCES IN THE PARENT UPON THE DEVELOPMENT OF SCROFULOUS CHILDREN.—In a recent address by Dr. Evory Kennedy, before the Dublin Obstetrical Society, the proposition was put forward that scrofula is affected by the surroundings and circumstances which modify the human organism, his views being founded upon the fact that "all organizations are not merely variable but varying." The following case was mentioned as an interesting illustration of the manner in which external circumstances may lead to the production of those modifications of the human organism which we collectively term "scrofula," and of a return to the normal condition when the circumstances which had produced the changes had passed away.

A peasant, whose family was liable to scrofulous modification, as seen markedly in his sister and family, married a woman free from any taint of scrofula, and had two children, who presented none of the characteristics known as scrofula. After their birth the father had an attack of rheumatic fever, which left him with an injured heart, and consequently in straightened circumstances from his crippled condition as a bread-winner. During this period of nipping poverty two other children were born, whose clumsy fingers, thick joints, tumid *alæ nasi* and upper lips, together with a strong tendency to glandular enlargements, mark the scrofulous diathesis. After this time the mother had an annuity left her, which once more placed them in comparative plenty. Two more children were born, approaching the healthy type of the two eldest, and comparatively free from the characteristics of the middle pair. When the family are gathered together, the history of the married life of the parents can be read in the physiques of their offspring.—*Br. Med. Jour.*

TREATMENT OF DIPHThERIA AND SCARLET FEVER.—In a late number of the "*Jahrbuch f. Kinderheilkunde*," Meyer describes his treatment of diphtheria with ice. Even when the children are very young and with infants under one year, he has them fed with small bits of ice, which are allowed to dissolve in the throat. In addition, he pours ice water on the tongue every few minutes. The ice should be very clean, and the artificial kind is the best. In very severe cases the external use of cold in the form of ice cravats is very suitable. It is also stated that under this treatment the fever generally yields, the membranes are thrown off, general infection of the system does not follow, and the disease rarely extends into the larynx. He employed cool baths in diphtheria to moderate excessive fever in one case, but in scarlet fever he found it generally of great advantage. When, however, the temperature exceeds 102.5° Fahr., tepid baths, varying between 71° and 81° Fahr., are to be used, and they may have as low a temperature as 64° Fahr. After the bath the temperature of the body remains reduced for a number of hours.—*Allg. Wien. Ztg.—Clinic.*

KANSAS CITY MEDICAL JOURNAL.

E. W. SCHAUFFLER, M. D., EDITOR.

EDITORIAL.

THE JOURNAL FOR 1875.

As our readers have already observed, the JOURNAL has changed its garb, returning to the form in which it was issued for three years. For the present it will appear but once in two months, each number containing about forty pages of reading matter. Although the fashion, among medical periodicals, of late, has been to increase the frequency of their issues, the monthlies becoming semi-monthlies, and the semi-monthlies, weeklies, it does not appear that the value of the Journals is, in many cases, increased. In one sense, the subscriber gets more for his money; he receives a larger number of pages of reading matter; in another sense he gets less, for he must wade through a good deal of rubbish to reach the truly valuable material.

In view of the fact that he is again left to the sole management of the JOURNAL, and that other duties are accumulating on his hands, the Editor has determined to return to the former plan of a bi-monthly issue.

He believes that by thus being enabled to devote more time and labor to each number, he will succeed in increasing the intrinsic value of the Journal and do greater justice to himself and to his readers.

A STEP IN ADVANCE.

The Faculty of Rush Medical College, Chicago, are entitled to the thanks of the profession for the advanced ground which they have just taken in the matter of making appointments of Lecturers in their school. The position of Lecturer on Obstetrics in the Spring Faculty was to be filled, and, instead of pursuing the ordinary course (still, so far as we know, in vogue in every

school in the land), of looking around amongst the friends and relatives of the Faculty for some good fellow not altogether incompetent for the place, or discussing, in solemn conclave, the question of who, if appointed, could bring the most influence to bear in favor of the school, it was determined to throw the matter open to competition, as follows :

Those gentlemen who applied for admission to this competitive exercise, or *Concours* (to use the French term), had subjects assigned to them and one week granted in which to prepare a lecture. Ten competitors were entered, the lectures being delivered at the College on three consecutive evenings. The lectures are declared all of them to have been of a very high order. Four of the lecturers were ranked alike, and were now subjected to this further and final test: "Titles of subjects were written upon separate slips of paper, placed in a hat, and from this hat the four selected at the previous trial were required to draw each in his turn, a slip, and, announcing the inscribed title, to proceed at once to lecture upon the subject thus assigned to him by dame Fortune."

The successful competitor proved to be Dr. E. W. Sawyer, the subject of whose first lecture was *Post-partum Hemorrhage*, and of his second, *The Anatomy of the Female Pelvis and its Deformities*.

The principle vindicated in this method of making appointments is an important one. The *Concours* is a familiar institution among aspirants to medical honors in Europe, but, so far as we know, it has never been established in this country. We are proud that a Western medical college should have been the first to introduce this practice.

MISCELLANY.

COLLEGE OF PHYSICIANS AND SURGEONS—COMMENCEMENT DAY.—The closing exercises of the Sixth Session of the Kansas City College of Physicians and Surgeons, will take place one day earlier than heretofore announced, to-wit: On Tuesday evening, March 2d. There are several candidates for the degree. Prof. E. W. Schauffler will deliver the address to the graduating class. Professional brethren from the city and surrounding coun-

try who can make it convenient to attend will be cordially welcomed. This change of time will enable members of the Kansas City District Med. Society, which meets on the day following, to be present at the college exercises, if they choose, without much additional loss of time.

S. S. TODD, M. D., *Pres't of the Faculty.*

ESSAYS TO BE READ AT THE NEXT MEETING OF THE KANSAS CITY DISTRICT MEDICAL SOCIETY.—At the next Quarterly meeting of the District Society, which is to be held in this city on Wednesday, March 3d, the following essays may be expected: On *Chronic Pulmonary Consolidations of Inflammatory Origin*, by Dr. T. B. Lester; on *Infantile Diarrhœa*, by Dr. B. F. Records; on *Scarlatina*, by Dr. M. A. Bogie. Drs. Johnson, of Platte, and Brookhardt, of Cass, have not yet informed the Secretary of their subjects. It is expected that Dr. Marsh of Liberty, will present a paper on *Cerebro-Spinal Fever*, and Dr. Wilson of Weston, one on *Inebriety and its Treatment*, both of these gentlemen having been prevented from filling their appointments at the last meeting.

CUBEBS IN DIPHTHERIA.—Dr. Reignier, of Surgeres, reports in *La France Medicale* (May 16), a case of nasal diphtheria in a boy, aged ten years, which he, two years previously, had successfully treated by the administration of a gramme of cubebs morning and evening; at the same time supporting the patient's strength, which was at the lowest ebb. There was an enormous quantity of albumen in the urine; and it was with the object of checking this drain from the system, that Dr. Reignier, calling to mind the action of cubebs on the kidneys, prescribed that drug, with a success which he was far from hoping for. He has since treated seven other cases of nasal diphtheria in a similar manner, and with a like success; and, therefore, thinks that he may now, without rashness, recommend its adoption. It would be prudent, he adds, to give the cubebs in these severe cases before the albumen made its appearance in the urine.—*Irish Hospital Gazette.*

... improvements in the healing
~~are of which~~ they appear to be practically ignorant. As an aid to diagnosis and prognosis, we would, however, recommend to their notice an ingenious instrument called the clinical thermometer, whose precise indications of temperature far exceed in value those expressed by the inexact phrases "skin cool" (p. 167), "skin dry but not hot" (p. 29), "skin dry and warmer than natural" (p. 27), etc., etc. Conscience forbids us to conceal any longer from Prof. Davis that a modified form of the antique magnifying-glass (with

THE PHYSIOLOGICAL ACTION OF JABORANDI.—Drs. Sydney Ringer and Alfred Gould of London, have recently experimented upon the action of this new drug. It is found in the interior of Brazil and has been brought thence by Dr. Continho. It consists of the leaves and small branches of a shrub that has a powerful aromatic odor. The dose is from thirty to ninety grains, infused in boiling water. These gentlemen confirm, in a great measure, the statements of M. Rabuteau as to its action on the skin, and they sum up their results as follows:

1. In three out of four cases, jaborandi causes profuse perspiration.
2. Jaborandi acted as a sialogogue, but was less certain in this respect than its diaphoretic effect.
3. In one case it increased the bronchial secretion.
4. It considerably accelerated the heart's action.
5. The temperature fell during its use.

Mr. Martindale, contrary to some high authorities, does not believe that jaborandi is composed of the leaves and stems of the pilocarpus pinnatifolius, and experiment with the latter seems to confirm his opinion.—*The Practitioner*.

THE ACTION OF MORPHIA SUBCUTANEOUSLY INJECTED.—From one thousand experiments made upon himself by Chouppe (*Gazette Med.* No. 35, 1874) the conclusion was drawn that morphine injected *in loco dolenti* develops its anæsthetic action in two to two and a half minutes earlier than when injected at a distant place. Pain ceased sooner after direct than after general application. A further direct proof of the local anæsthetic action of this agent was derived from the employment of concentrated solutions. While an injection of distilled water and a weak solution of morphia (1-150) caused sharp pains at the point of insertion, strong solutions (1-50 or 1-30) cause no perceptible pains. The indication therefore is to make the injections at the seat of pain and to use concentrated solutions.—*Allgem. Wiener Med. Zeit.*

GREAT INVENTION.—Lloyd, the famous map man, who made all the maps for General Grant and the Union army, certificates of which he published, has just invented a way of getting a relief from steel so as to print Lloyd's Map of American Conti-

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CROTON-OIL PAINT.—Dr. John W. Corson recommends as a counter-irritant in pleurisy and various other affections, the following, which he names croton-oil paint.

No. 1. Milder croton-oil paint.

R.—Olei tiglii, ℥j;
Ether. sulphur. fort. ℥ij;
Tinct. iodin. ℥v.—M.

Two or three coats at a time to be applied with a camel's hair brush, over a small surface once a week.

No. 2. Stronger croton-oil paint.

R.—Olei tiglii, ℥ij;
Ether. sulphur. fort. ℥iv;
Tinct. Iodin., ℥ij.
Potass. iodid. ℥j.
Iodinii, gr. x.—M.

S.—Paint as above.

PHOSPHORUS, which are added in such proportions as to render FOUGERA'S COD LIVER OIL FIVE TIMES STRONGER and more efficacious than pure Cod Liver Oil.

Fougera's Vermifuge.

(COMP. DRAGEES OF SANTONINE.)

Santonine, the active principle of *Semen contra*, (European Wormseed,) occupies the first rank among the anthelmintic remedies. In this preparation the Santonine is combined with a purgative agent under the form of a sugar coated pill, and thus forms a pleasant and efficient medicine.

REVIEWS.

CLINICAL LECTURES ON VARIOUS IMPORTANT DISEASES. BEING A COLLECTION OF THE CLINICAL LECTURES DELIVERED IN THE WARDS OF MERCY HOSPITAL, CHICAGO. By NATHAN S. DAVIS, A. M., M. D., Professor of Principles and Practice of Medicine and Clinical Medicine in Chicago Medical College. Edited by FRANK H. DAVIS, M. D. Second Edition. Philadelphia: Henry C. Lea. 1874. Pp. 283.

This Journal expressed itself on a former occasion as to the value (?) of the Clinical Lectures above named, to the profession at large. It is but just to the present publishers to state that the gross errors of the Chicago edition, the hodge-podge of English, latin and hog-latin in the formulas, have been corrected. We cannot withhold from our readers the following just criticism of the *Philadelphia Medical Times*:

"Our space is too limited to permit us to point out to the Doctors Davis, for their own benefit as well as that of their *students* and *patients*, many of the improvements in the healing art of which they appear to be practically ignorant. As an aid to diagnosis and prognosis, we would, however, recommend to their notice an ingenious instrument called the clinical thermometer, whose precise indications of temperature far exceed in value those expressed by the inexact phrases "skin cool" (p. 167), "skin dry but not hot" (p. 29), "skin dry and warmer than natural" (p. 27), etc., etc. Conscience forbids us to conceal any longer from Prof. Davis that a modified form of the antique magnifying-glass (with

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